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*Published in:*  
American Journal of Agricultural Economics

*DOI:*  
[10.1111/ajae.12105](https://doi.org/10.1111/ajae.12105)

**IMPORTANT NOTE:** You are advised to consult the publisher's version (publisher's PDF) if you wish to cite from it. Please check the document version below.

*Document Version*  
Publisher's PDF, also known as Version of record

*Publication date:*  
2020

[Link to publication in University of Groningen/UMCG research database](#)

*Citation for published version (APA):*

Belissa, T., Lensink, R., & Winkel, A. (2020). Effects of Index Insurance on Demand and Supply of Credit: Evidence from Ethiopia. *American Journal of Agricultural Economics*, 102(5), 1511-1531.  
<https://doi.org/10.1111/ajae.12105>

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# EFFECTS OF INDEX INSURANCE ON DEMAND AND SUPPLY OF CREDIT: EVIDENCE FROM ETHIOPIA

TEMESGEN BELISSA, ROBERT LENSINK, AND ANNE WINKEL

Index-based insurance offers a climate risk management strategy that can benefit the poor. This article focuses on whether adopting index insurance improves access to financial markets and reduces credit rationing, using empirical analyses focused on Ethiopia. With different identification strategies, including a newly developed method that leverages the varying availability of index insurance across areas, the authors control for potential selection biases by forecasting potential insurance adopters; they apply a cross-sectional double-difference method. Credit rationing can take the form of either supply-side quantity rationing, in which case potential borrowers who need credit are involuntarily excluded from the credit market, or demand-side rationing, such that borrowers self-select and voluntarily withdraw to avoid transaction costs and threats to their collateral. By differentiating supply-side and demand-side forms and employing a direct elicitation method to determine credit rationing status, this study reveals that 38% of sample households are credit constrained. The preferred estimation techniques suggest that index insurance significantly reduces supply-side rationing.

*Key words:* Credit Rationing, Ethiopia, Index Insurance; Smallholders.

*JEL codes:* D82, G21, G22, O13, O16, O55.

Index-based insurance (IBI) is an innovative hedging instrument that can mitigate the risks of drought or seasonality-based weather variations due to climate change. As an attractive feature of this innovation, the insurance payout occurs when an objective index falls below (or exceeds, depending on the criterion) a threshold. The index usually is based on a measure of the intensity of rainfall or direct yield measures for a specific geographic zone covered by the insurance contract (Carter et al. 2017). Ideally, the index correlates closely with insured losses, is objectively quantifiable, and is publicly verifiable, such that it cannot be manipulated by the

insurer or the insured (Barnett, Barrett, and Skees 2008; Zant 2008).

Although the uptake of IBIs remains low<sup>1</sup> (Cole et al. 2012, 2013; Hill, Hoddinott, and Kumar 2013; Dercon et al. 2014; Takahashi et al. 2016) and demand is generally sluggish (Carter et al. 2017), several studies point to its potentially substantial income, investment, or wealth effects (Janzen and Carter 2019; Elabed and Carter 2014; Karlan et al. 2014). For example, IBI adoption could induce households to make more prudent investments or manage their consumption risk better (by stabilizing savings or accumulating assets), which then could protect them from

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<sup>1</sup> The low uptake of IBI might stem from the so-called basis risk, which arises due to the imperfect correlation between computed indices and actual losses (Cummins, Lalonde, and Phillips 2004; Jensen, Mude, and Barrett 2018). Especially in developing countries, where terrestrial weather stations are sparse, the discrepancy between losses indicated by the index and actual losses realized at the farm level is high (Clarke et al. 2012).

*Amer. J. Agr. Econ.* 102(5): 1511–1531; doi:10.1111/ajae.12105

Published online June 11, 2020

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sliding into poverty traps (Barnett, Barrett, and Skees 2008). Moreover, because IBI unlinks loss assessments from individual behavior, it can avoid moral hazard and adverse selection problems (Barnett, Barrett, and Skees 2008; Skees 2008), which also might improve credit access. However, virtually no empirical evidence exists regarding how IBI adoption affects the demand for and supply of credit. This article offers initial insights into the impacts of IBI on credit rationing, using data from a drought-prone area in the Rift Valley zone of Ethiopia.

Despite the lack of empirical evidence, some studies offer initial insights into the potential effects of IBI on credit rationing. In a framed field experiment in China, Cheng (2014) studies the effect of offering IBI to households that voluntarily withdraw from the credit market; all the study participants participate in both control and treatment games. The results demonstrate that more than half of the farmers decide to apply for credit when IBI is available, and roughly two-thirds of credit diverters choose to use their loan for productive investments rather than consumption. This behavior could be explained in several ways, with the recognition that IBI reduces production risk, but the risk associated with using the loan for consumption remains constant or even might increase. In addition, Giné and Yang (2009) randomly offered maize farmers in Malawi either a loan or a loan with insurance, which indemnifies them if rainfall is insufficient. The insurance could be bought at an actuarially fair premium. Although they expected that farmers would prefer the insured loan, demand for the basic loan was 13% higher than that for the insured loan. To explain why, these authors propose that the limited liability clause of the basic loan provided an implicit form of insurance. However, in contrast with the current study, Giné and Yang (2009) do not address the impact of a stand-alone insurance product. As Carter, Cheng, and Sarris (2016) suggest, a stand-alone insurance product provides no additional benefits to farmers with limited collateral. If no formal insurance is available, farmers with high collateral might be the only ones who choose not to borrow, because they do not want to put their collateral at risk.

Other studies focus on the impact of IBI on credit supplies, with conflicting results. That is, some research suggests that IBI relaxes supply-side constraints and quantity rationing, because lenders tend to lower interest rates and lend more to insured clients due to the reduced default risk (Mahul and Skees 2007;

Giné and Yang 2009; McIntosh, Sarris, and Papadopoulos 2013). But other studies suggest that insurance could decrease both demand for and supply of credit. Because IBI contracts sometimes are unable to trigger payouts, even if the insured incurs significant yield losses due to weather risk, an inability to repay the loan can lead to loan defaults, because cash outflows go to paying the premium. Alternatively, a stand-alone insurance product could increase the minimum welfare level to which defaulting households can retreat. In this situation, incentives to repay diminish, because the welfare costs of defaulting are lower. Lenders who take loan default potential into account thus might limit credit supply in areas where IBI is available (Banerjee 2000; Clarke and Dercon 2009; Farrin and Miranda 2015).

With this study, we seek to provide actual, empirical evidence of the impact of IBI on demand-side (i.e. risk and transaction cost rationing) and supply-side credit rationing. The insurance contract is a stand-alone product, for which indemnities are paid directly to farmers. Hence, the insurance intervention in Ethiopia that we study is not an insurance–credit interlinked contract. As the majority of smallholders lack any valuable collateral to offer, there is no real collateral at risk for most formal borrowers.<sup>2</sup> Thus, the intervention seems unlikely to exert any considerable impact on risk rationing, because insurance cannot make borrowers who are insured by a zero collateral default clause borrow more nor should it have any impact on transaction cost rationing. Therefore, we expect the adoption of IBI mainly reduces supply-side, rather than demand-side, rationing.

We use different identification strategies, including a newly developed hybrid method. The preferred methods suggest that index insurance has a large, significant effect on decreasing supply-side credit rationing. But the impact of IBI uptake on demand-side rationing is statistically insignificant. To establish these findings, we start in the next section with a description of the study setting, how IBI works, and the sampling procedure. Next, we explain the method used to determine credit rationing and provide estimates of credit rationing in our sample. We present a new, hybrid method based on a double-difference (fixed effects) technique to estimate the impact

<sup>2</sup> In the Online Appendix we provide more information about the nature of the insurance contract and liability rules in Ethiopia.

of IBI on credit rationing; we outline how it helps address issues with traditional regression techniques. Finally, we note some limitations of our hybrid method, offer robustness analyses, and conclude with relevant insights.

### Study Setting, Data, and Sampling

This study took place in the central Rift Valley zone of the Oromia regional state in south-eastern Ethiopia. The area is characterized by plain plateaus and lowland agro-ecology. The pattern and intensity of rainfall exhibits considerable spatial and temporal variation, with a bimodal distribution. Rainfall seasons are from May to August and during October and November. However, moisture stress and drought frequently cause devastating crop failures, rampant livestock mortality, and herd collapse (Dercon, Hoddinott, and Woldehanna 2005; Takahashi et al. 2016). Major droughts in the area occurred in 2015–16, following historical drought trends during 1973–74, 1983–84, 1991–92, 1999–2000, 2005–6, and 2011–12 (Dercon 2004; Takahashi et al. 2016). Households are smallholders and subsistence farmers who often face drought-induced income shocks that translate into erratic consumption patterns (Dercon and Christiaensen 2011; Biazin and Sterk 2013). Formal risk management mechanisms are inaccessible, requiring informal methods. Ex post coping mechanisms include reducing the frequency of meals, distress livestock sales, halting the purchase of fertilizer and improved seeds, forcing pupils to withdraw from school for casual labor, renting land and family labor to local landlords, and seeking wage-based employment on floriculture farms held by foreign investors. These mechanisms are costly and limited in scope (Dercon 2004).

### IBI in the Study Area

In 2013, the Japan International Cooperation Agency (JICA) and Oromia Insurance Company (OIC) jointly implemented IBI for crops in the Rift Valley zone of Ethiopia to improve the resilience of households, in the face of climate change. The product was designed by CelsiusPro Ag and Swiss Re using satellite weather data with  $10 \times 10$  km grids for the period 1983–2012. It was implemented in five districts: Boset, Bora, Ilfata, Adamitullu-

Jido-Kombolcha (AJK), and Arsi Negele. The selection of which kebeles<sup>3</sup> to cover in each district reflected several criteria, including rainfall shortages, the relevance of the area for crop production, and food security. The specific selection process worked as follows: first, before the initial intervention in 2013, the OIC, JICA, and Ethiopian Ministry of Agriculture discussed and identified districts in which drought shocks are common. Most of these districts are located in the Rift Valley zone. Second, the three organizations entered into focus group discussions with representative farmers from each kebele within each selected district. From these discussions, they identified kebeles with severe drought experience in the past. Third, the financial support that JICA allotted for the 2013 weather index insurance intervention was not adequate to cover all the identified drought-prone kebeles at the same time, so the kebeles that would be subject to the first intervention in 2013 versus during later interventions were selected through discussion among the three organizations.<sup>4</sup>

The product is marketed and sold twice per year, in April and September, which are the months preceding the two rainy seasons. It provides coverage against losses during the seedling and flowering stages of crop growth. Major targeted staple food crops include maize, wheat, barley, and teff. During each sales period, a household that decides to buy IBI pays a premium of ETB<sup>5</sup> 100 per policy. The payout is determined according to the level of normalized difference vegetation index, measured using satellite data for a specified period. The exact trigger and exit level differ for each kebele and in each season. A 20% markup on the actuary fair premium also is included in the contract, to cover selling and administrative costs (loading factor).<sup>6</sup> Selling costs are incurred by the sales agents, such as

<sup>3</sup> A kebele is the smallest administrative unit, within districts (Woreda), in Ethiopia.

<sup>4</sup> We tried to obtain details about how the kebeles to be served first were selected. The official answer of OIC was that it was random, but the organization could not specify how the randomization was conducted. It seems more likely that the final selection was based on informal criteria, influenced by the group discussions.

<sup>5</sup> ETB (Ethiopian Birr), 1USD = 32 ETB.

<sup>6</sup> This mark-up rate is relatively low compared with other products in developing countries, which reflects a practical challenge: a higher mark-up rate that takes all catastrophic risks and uncertainty into account would increase the product price too high, relative to farmers' reservation prices. Thus, the product does not meet safe minimum quality standards, in the sense that high insurance prices might prompt farmers to prefer self-insurance (Carter et al. 2017).

cooperative unions. The actuarially fair premium of the insurance product is ETB 100 per policy for a maximum payout of about ETB 667 to cover full losses. The 20% markup is paid by JICA, so farmers only pay the actuarially fair premium of ETB 100 per policy. According to OIC sales records, before September 2014 (start of the data collection period), approximately 5,000 households in more than thirty kebeles across five districts had purchased IBI; plans were in place to expand into many other adjacent kebeles in each district. The intervention plan in subsequent periods involved intensifying coverage of the kebeles in the five districts, then expanding into other districts. Our newly developed hybrid identification strategy, which we explain subsequently, reflects these expansion plans.

### *Sampling*

We used a multistage, random sampling technique with probability proportional to size. Concretely, we selected three districts (Bora, AJK, and Arsi Negele) out of the five districts covered by the IBI project. Then we identified a random sample of kebeles covered by IBI in these three districts. From the AJK district, we also selected two kebeles (Qamo Garbi and Desta Abijata) where IBI was not yet available.<sup>7</sup> Noting our available budget, we decided to survey approximately 1,200 households, selected proportional to the sample size of the study from all kebeles identified. We also oversampled households from the treatment kebeles to ensure sufficient actual adopters in the sample. That is, we took larger samples in the treatment kebeles than the control kebeles, proportional to the size of the kebeles in both groups. We used random sampling within the treatment and control kebeles. For each kebele, we randomly selected the sample from a roster of farmers; for simplicity, we adopted a systematic random sampling technique and randomly selected the first farmer from the roster then the next  $n^{\text{th}}$  farmers from the list.

<sup>7</sup> These two kebeles had been identified for potential future interventions. The presence of just two kebeles in the control condition reflects our assumption that between-kebele variation explains only minimal differences across individuals. However, as local administrative boundaries in Ethiopia, kebeles have varying size, and the two kebeles we selected to serve as controls both contain large populations, such that the samples taken from them constitutes about one-third of the total respondents, proportional to the total households, in both groups of kebeles.

Prior to our study, JICA and OIC organized product promotion campaigns in both the treatment and expansion kebeles to enhance uptake (and future uptake). In both the treatment and control kebeles, an estimated 70% of the farmers attended a product promotion meeting. Such product promotion campaigns are common tactics to roll out new insurance products.

The final sample consists of 1,143 households: 812 from the treatment and 331 from the control kebeles. Of the 812 households from the treatment kebeles, 459 adopted and 353 did not adopt IBI over the 2013–14 period. This relatively high percentage of adopters can be explained by the well-attended product promotion campaigns. As table 1 indicates, an average household in the treatment kebeles that bought IBI was headed by a 39-year-old man who had about two years of formal education. The average number of family members is seven, four of whom have reached productive age, with three who are either preschool children or aged dependents. On average, these households travel approximately two hours to access public services, including market centers and financial institutions. Although some differences arise with households that do not buy insurance in the treatment kebeles compared with households in the control kebeles, these differences are small. Yet, as will also be shown in table 4 below, this does not imply that treatment and control kebeles are similar in terms of various poverty indicators.

### **Methodology**

If rural financial markets were perfectly competitive, with symmetric information and costless enforcement, lenders could arrange conditional credit contracts according to borrowers' behaviors. However, rural financial markets in developing countries often feature information asymmetries, leading to moral hazard and adverse selection problems, as well as higher transaction costs associated with loan monitoring and contract enforcement (Jaffee and Russell 1976; Stiglitz and Weiss 1981; Vandell 1984; Bester 1985; Williamson 1986; Besanko and Thakor 1987; Lensink and Sterken 2002; Boucher, Carter, and Guiringer 2008). These classic incentive problems make conditional credit contracts restrictive and infeasible (Dowd 1992; Ghosh,

**Table 1. Sample Characteristics**

Variables	Age	Gender	Education	Dependency ratio	Distance from market
Treatment kebeles (adopters)	39.4	0.8	2.5	0.5	1.7
Treatment kebeles (non-adopters)	38.1	0.9	2.3	0.5	1.7
Control kebeles	37.8	0.8	2.3	0.5	1.5
t-statistic	-1.3	-2.6***	-2.4***	2.1**	-1.1
Observations	1,143	1,143	1,143	1,143	1,143

Note: The statistics in table 1 refer to the mean of adopters in the treatment kebeles; the mean of non-adopters in the treatment kebeles; and the mean for households in the control (expansion) kebele. The t-statistic refers to a comparison between Treatment kebeles (average) and Control Kebeles. \*\*\* $p < 0.01$ .

\*\* $p < 0.05$ . \* $p < 0.1$ .

Mookherjee, and Ray 2000; Lensink and Sterken 2001; Conning and Udry 2007).

Rural financial markets also typically are characterized by credit rationing, such that lenders set a ceiling and avoid extending additional credit, even if borrowers are willing to pay higher rates of interest. If borrowers are credit constrained, greater access to credit can facilitate investment. The ability to borrow also constitutes a major source of private insurance against consumption fluctuations that arise due to idiosyncratic shocks in rural areas though, so credit rationing can have far-reaching, adverse welfare consequences, including decreased consumption smoothing (Morduch 1995; Zimmerman and Carter 2003) and limited investments in high-risk, high-return inputs or applications of expensive resources such as chemical fertilizer (Beegle, Dehejia, and Gatti 2003; Guarcello, Mealli, and Rosati 2010; Hill, Hoddinott, and Kumar 2013; McIntosh, Sarris, and Papadopoulos 2013). Credit-constrained households might be forced to lower their expenditures on nutrition, which may lead to persistent health outcomes, such as stunted child development (Islam and Maitra 2012). Reducing access to credit, as a tool to smooth household income fluctuations, also can prompt school dropouts, due to the need for increased child labor on landlord farms. This step lowers the level of human capital development (Beegle, Dehejia, and Gatti 2003; Guarcello, Mealli, and Rosati 2010). Exposure to repeated downturns and inaccessible credit thus reinforces vulnerability and perpetuates poverty by shaping both the behaviors of and outcomes for the poor.

Credit rationing can take the form of either supply-side quantity rationing, in which case potential borrowers who need credit are involuntarily excluded from the credit market, even though they would be willing to pay higher

interest rates, or demand-side rationing, such that borrowers self-select and voluntarily withdraw from the credit market to avoid transaction costs and threats to their collateral (Bester 1987; Boucher, Carter, and Guirking 2008). Demand-side credit rationing thus includes both risk and transaction cost rationing. Risk rationing is prevalent in the absence of insurance markets when lenders shift contractual risks to borrowers; borrowers respond by voluntarily withdrawing from the market, even though the value of their collateral is sufficient to qualify them for a loan contract (Boucher and Guirking 2007; Boucher, Carter, and Guirking 2008). Similarly, transaction cost rationing arises when the costs of waiting for, processing, and administering the loan are too high for borrowers (Boucher, Guirking, and Trivelli 2009). Noting these several definitions of credit rationing, we use a direct elicitation method (DEM; Boucher, Guirking, and Trivelli 2009) to identify the credit rationing status of each household. With this method, we can identify credit-constrained households according to their decision to borrow and the lender's decision to supply credit.

Therefore, the credit rationing module starts by asking whether the respondent has applied for a formal loan in the past five years. If so, it asks whether the application has been accepted. Households that have not applied for a formal loan indicate their reasons for not applying. According to their responses, all households can be categorized into one of four mutually exclusive groups: credit unconstrained, quantity (or supply-side) rationed, risk rationed, and transaction cost rationed. Households that apply for formal loans and receive them are categorized as unconstrained. However, if households applied for (more) credit at the prevailing interest rate and their application was rejected, they are classified as quantity rationed. If households

have not applied for a formal loan in the past five years, because the bank branch is too far from their homes or the application procedure involves too much paperwork and waiting time, we categorize them as transaction cost rationed. If instead households do not apply for loans because they do not want to offer their house or other assets as collateral that might be taken by the bank, we consider them risk rationed. Some households that are able to borrow do not apply because they do not need credit; they are credit unconstrained. Finally, households that would have applied for loan, had they known the bank would lend to them, are another group of supply-side rationed households. We combine the risk- and transaction cost-rationed households into a group of demand-constrained households; then we combine the demand-constrained and supply-constrained households into a larger group of credit-constrained households. Figure A1 in Appendix I presents a tree diagram of questions leading to the final categorization of each household.

### *Credit Access in the Sample*

Using the DEM, we identify the credit rationing status of each household in our sample. Table 2 summarizes the results. Approximately 38% of the households are credit constrained, of which 20.5% are quantity constrained and 18% are demand constrained.<sup>8</sup> The table also differentiates kebeles with and without access to index insurance, revealing that the percentage of households that are credit constrained is higher in the kebeles with access to index insurance than in the kebeles without it. The same trend holds for the percentage of households that are quantity constrained. However, we hasten to add that this finding does not necessarily mean that access to index insurance (like an intention to treat analysis) leads to more credit rationing.

### *Specification of the Regression Model*

To clarify our approach, we specify the following fixed effect model:

$$(1) \quad y_i = \beta_0 + \beta_1 I_i + [\alpha^t D_i^t + \alpha^w D_i^w + \mu_i],$$

<sup>8</sup> This credit rationing level (38%) is relatively lower than that found in Peru (55.4% in 1997; 43.6% in 2003; Boucher, Guirkiner, and Trivelli 2009). Quantity rationing accounts for 20.5% in our study, versus 36.6% in 1997 and 10.4% in 2003 in Peru. Similarly, demand rationing is 17.8% in our case but was 18.8% in 1997 and 33.2% in 2003 in Peru.

where the dependent variable  $y$  refers to a farmer-level outcome (credit rationing) for farmer (observation)  $i$ ;  $I_i$  is a binary variable that takes a value of 1 if  $i$  is insured;  $D_i^t$  is a binary variable equal to 1 if  $i$  is the type who would endogenously buy insurance, such that it reflects a fixed effect for those who would buy, irrespective of whether insurance is available;  $D_i^w$  refers to a fixed effect for treatment kebeles, which is also a binary variable, equal to 1 if  $i$  is in a kebele where index insurance is available, indicating the program was endogenously placed; and  $\mu_i$  is an error term. Note that  $I_i$  equals the interaction term  $[D_i^t * D_i^w]$ , so Equation 1 is equivalent to a double difference model (as we explain subsequently when detailing the preferred method).

Our main aim is to obtain an unbiased estimate of  $\beta_1$ , which may be hindered by self-selection or program placement biases. Therefore, before presenting our preferred approach, we briefly discuss the nature of these biases, as they arise under standard estimation techniques. Table 3 presents the results of these estimates. To start, we run naïve ordinary least square (OLS) regressions with and without controls, using only data from the treatment kebeles where the program was endogenously placed, which implies that from Equation 1, the terms  $\alpha^t D_i^t + \alpha^w D_i^w$  drop out. By using data from the treatment kebeles only, we compare credit rationing by farmers who bought insurance against the credit rationing of their neighbors who deliberately decided not to take part in the insurance program. An OLS estimator in this case is likely biased due to self-selection. Self-selection based on unobservable variables is the problem; adding explanatory variables may control for selection on observables. The OLS estimators of  $\beta_1$  would be downwardly biased if participants in the insurance program are previously more credit constrained, even before they have the option to buy insurance, and if credit constrained farmers are more willing to participate in the insurance program. This bias is particularly likely for supply-side credit constraints, in that intuitively, supply-side-constrained farmers (with observable and unobservable characteristics that make them credit constrained) would want to buy insurance, because they lack the option to use credit to protect themselves against crop loss, such as due to a drought. Regarding

**Table 2. Credit Rationing in the Sample (Percentages)**

Variables	Entire Sample	Treatment kebeles	Control kebeles	t-Test (comparing treatment with control) <i>p</i> -values
Credit rationed	0.382	0.400	0.338	0.05 (0.26)
Quantity rationed	0.205	0.228	0.148	0.002 (0.05)
Demand rationed	0.178	0.172	0.190	0.47 (0.66)
Number of observations	1143	812	331	

*Note:* In the last column, comparing the means for the treatment and control kebeles, the first value is the *p*-value using normal standard errors; the second (in brackets) is the *p*-value based on cluster-robust standard errors (clustered at kebele level).

demand-side credit rationing, the nature of the bias is less clear in advance; in this case, farmers voluntarily withdraw from the credit market. To learn about the nature of the selection bias on unobservables, we can compare  $\beta_1$  for the OLS estimates with and without controls, assuming that the extent of selection on the observed explanatory variables provides an indication of the extent of selection on the unobservables. It appears that  $\beta_1$  is insignificant for the different credit rationing variables in all OLS regressions, with and without controls, yet it is more negative for the estimates with controls, compared with those without controls, for overall credit rationing and for supply-side credit rationing. The decrease in  $\beta_1$  for supply-side credit rationing is notably big, from +0.007 to −0.047, which suggests that the OLS estimator of  $\beta_1$  for supply-side credit rationing is biased downward, and the OLS estimate of  $\beta_1$  underestimates the supply-side credit constraints, thereby reducing the effects of insurance. For demand-side credit rationing, the increase in  $\beta_1$  from −0.021 to 0.024 is

insignificant. Except for kebele fixed effects, none of the explanatory variables in the outcome equation for demand-side credit rationing is significant, so we are hesitant to draw conclusions regarding the nature of the bias of the estimate of  $\beta_1$  in this case.

As further evidence that the OLS estimator of  $\beta_1$  is biased downward, especially for supply-side credit constraints, we also compared the naïve OLS estimates with an estimator that controls (partly) for endogenous self-selection. With an endogenous treatment regression model (ETRM), we assume a linear model for the outcome and allow for correlation structures between unobservable variables that affect the treatment and outcomes (see Heckman 1976, 1978; Wooldridge 2010). The estimation of this endogenous binary variable model includes equations for the outcome variable and the endogenous treatment (adoption of index insurance). The endogenous treatment model suggests that IBI lowers overall credit rationing, but the impact is insignificant (yet more negative than the OLS estimates). It also indicates a significant, negative effect on supply-side credit rationing, such that uptake of

**Table 3. Impact of IBI on Credit Rationing**

	OLS1	OLS2	ETRM	ITT	LATE
Overall credit rationing	−0.014 (0.05)	−0.022 (0.04)	−0.132 (0.10)	0.06 (0.05)	0.11 (0.09)
Supply-side credit rationing	0.007 (0.04)	−0.046 (0.04)	−0.121 <sup>†</sup> (0.06)	0.08 <sup>†</sup> (0.04)	0.14 <sup>†</sup> (0.06)
Demand-side credit rationing	−0.021 (0.05)	0.024 (0.04)	−0.008 (0.07)	−0.02 (0.04)	−0.03 (0.07)
Controls	No	Yes	Yes	No	No
Treatment equation	No	No	Yes	No	No

*Note:* The sample is the treatment kebeles for OLS1, OLS2, and ETRM but the treatment and control kebeles for ITT. OLS = ordinary least squares, ETRM = endogenous treatment regression models, and ITT = intention to treat estimator. Cluster-robust (kebele cluster) standard errors are in brackets. OLS1 is estimated without controls. The controls for the other regressions in the outcome equation are Age, Education (years), Family size, Extension contact, District1, District2, Dependency ratio, Dependents, Coop member, and kebele fixed effects (see Table A1). For overall credit rationing and supply-side credit rationing only *coop member* (negative sign) and kebele fixed effects are significant; for demand-side credit rationing, only kebele fixed effects are significant. The controls in the (endogenous) treatment (uptake) equation are specified in Table 6.

<sup>†</sup> Significant at 5%.



IBI reduces credit rationing by approximately 12%, whereas the effect on demand-side credit rationing remains insignificant. The substantial difference between the OLS regression results and the endogenous treatment model results imply that selection on unobservables probably has an important role.<sup>9</sup>

Considering the difficulty of ruling out selection biases using observations in the treatment kebeles only, we derive estimates using data from the control kebeles as well. A straightforward method to address selection biases is to conduct intention to treat (ITT) analyses. For Equation 1, this method would exclude  $\beta_1 I_i + \alpha' D_i^l$ . The ITT estimate is given by  $\alpha^w$ . Moreover, we could estimate the impact of actual uptake of insurance by using the treatment kebeles as an instrument to estimate  $I_i$ , which would give us the local average treatment effect (LATE). The main disadvantage of these estimators is that they only provide unbiased estimates if we ignore endogenous program placements and assume program placement was (nearly) random. However, as we have explained, the selection of treatment and control kebeles was not entirely random, so the ITT and LATE estimates may suffer from program placement bias. Table 4 compares the averages for the treatment and control kebeles in terms of population density<sup>10</sup> and poverty indicators, such as housing conditions,<sup>11</sup> being a beneficiary of a productive safety net program (PSNP), a wealth index,<sup>12</sup> livestock sizes,<sup>13</sup> and an indicator of access to non-farm employment. The table offers strong

evidence of program placement bias, because the treatment and control kebeles differ on various poverty indicators, including housing quality, PSNP dependence, wealth indexes, and the dependency ratio. The ITT and LATE approaches therefore are problematic and not appropriate. Yet, their estimates suggest that access to index insurance does not significantly affect overall or demand-side credit rationing. Surprisingly, in contrast with the OLS and ETRM results, the ITT and LATE estimates suggests that IBI access *attenuates* supply-side credit rationing.<sup>14</sup> However, these results logically appear more likely to measure differences between control and treatment kebeles than unbiased impacts of (having access to) insurance.

### *A New Hybrid Method: Double Difference in Space*

The final method we introduce is our newly developed hybrid approach, in line with a double-difference model. This approach can be summarized as follows:

1. We consider households (adopters and non-adopters of IBI) in areas where IBI is currently available (treatment kebeles).
2. We consider households in areas where IBI is not yet available, which represent potential future clients, according to the insurance company (control kebeles; the expansion area for the insurance company).
3. Using the sample of households in the treatment kebeles (where IBI is available), we estimate propensity to adopt and then conduct an out-of-sample forecast of “expected” adopters and expected non-adopters in the control kebeles (expansion area where IBI is not yet available). To conduct the out-of-sample forecasts, all variables in the equation used to estimate propensities to adopt in the treatment kebeles also need to be (and are) available for the expansion (control) kebeles.
4. We estimate the impact of IBI on credit rationing with a double-difference method in a cross-sectional framework, with three comparisons: (a) *predicted* adopters and *predicted* non-adopters in the treatment area (we use *predicted* adopters instead of *actual* adopters, because unobserved factors may both increase uptake and affect

<sup>9</sup> The advantage of the endogenous treatment model is clear: it provides, with certain assumptions, unbiased estimates of the impact in the case of endogenous selection. However, the reliability of this model depends on the variables used to determine the endogenous treatment. The reliability of the outcomes depends on whether the exclusion restrictions hold. It is debatable if the exclusion restriction holds in our case.

<sup>10</sup> We measured population density as the ratio of households' family size to land size. Households in areas that have dispersed population density may have more land, which may offer them better economic opportunities. We calculate the net size of land possessed or owned by the household from the details of land owned by the household adjusted for land rented in, rented out, sharecropped in, and sharecropped out. Comparing the treatment and control kebeles on the ratio of family size to land provides a good proxy for the economic implications of population density.

<sup>11</sup> We proxied housing quality with a variable that indicates whether the household owns an iron corrugated house. In Ethiopia, iron corrugated houses represent an improvement over thatch houses.

<sup>12</sup> To construct the wealth index, we use information about various plant asset compositions (i.e. farm implements, including harrows, plows, sickles, and spades, as well as household assets such as radios, televisions, or motorcycles) owned by households, together with the salvage value of each asset in our data set.

<sup>13</sup> Livestock owned by the household is measured in TLU, which is a standard unit.

<sup>14</sup> Note that the LATE is a transformation of the ITT estimator.

Table 4. Comparing Treatment and Expansion Region

Variables	Full sample	Treatment kebeles	Expansion kebeles	Mean difference	t-Stat
Population density	1.227 (1.105)	1.266 (0.042)	1.133 (0.890)	-0.133 (0.072)	-1.837
Housing quality	0.642 (0.480)	0.606 (0.489)	0.731 (0.444)	0.125 (0.031)	4.030***
Net salvage value of physical assets	7360.05 (10121.90)	7075.87 (7908.05)	8057.18 (14148.23)	981.31 (659.72)	1.487
Saving (ETB)	1256.99 (4805.23)	1414.40 (5217.10)	870.85 (3578.41)	-543.55 (313.09)	-1.736
PSNP dependent	0.117 (0.322)	0.087 (0.283)	0.190 (0.393)	0.103 (0.021)	4.953***
Livestock owned in TLU	9.634 (8.069)	9.759 (8.097)	9.329 (8.004)	-0.423 (0.526)	-0.816
Wealth index	0.546 (0.498)	0.478 (0.018)	0.713 (0.025)	0.235 (0.032)	7.408***
Dependency ratio	0.499 (0.202)	0.491 (0.007)	0.519 (0.011)	0.028 (0.013)	2.149**
Access to non-farm employment	0.388 (0.487)	0.400 (0.490)	0.356 (0.480)	-0.044 (0.032)	-1.377

Note: Standard deviations are in brackets. The cluster-robust standard errors are clustered at the iddir level. \*\*\* $p < 0.01$ . \*\* $p < 0.05$ . \* $p < 0.1$ .

credit rationing); (b) *predicted* adopters and *predicted* non-adopters in the control area, and (c) the differences of a with b.

Thus, our approach combines propensity scores with difference-in-difference (DiD) estimators to improve identifications when baseline data are not available.<sup>15</sup> To the best of our knowledge, no empirical studies have used this approach previously to estimate the impact of insurance.<sup>16</sup> This method aims to reveal the extent to which predicted adopters in the treatment area benefit more than predicted non-adopters in the treatment area, relative to the difference between predicted adopters and non-adopters in the control area.

The DiD elements of our approach are similar to those of a standard DiD, which would compare between years and participants and non-participants, whereas DiD in a cross-sectional setting compares program and non-program villages, and then target and non-target groups (e.g. Coleman 1999; Armendariz and Morduch 2010; Khandkar, Koolwal, and Samad 2010).<sup>17</sup> In our case, the DiD involves a comparison of the difference between the outcomes of farmers who take up insurance and those who do not, in areas where insurance is available, against the outcomes of farmers who (would like to) take up insurance and farmers who do not (want to) take up insurance in areas where insurance is not yet available. Table 5 provides an illustration. We consider kebeles where insurance is available (1) and kebeles without insurance access (2). In the former, we know who participates (takes up insurance, group A) and who does not (group C). In the latter, we identify a group that likely would participate if insurance were available (group D) and a group that probably would not participate even if insurance were available (group B).

<sup>15</sup> Note that DiD requires a parallel trends assumption, and propensity score matching assumes that selection is driven by factors that can be observed.

<sup>16</sup> Cecchi, Duchoslav, and Bulte (2016) use a somewhat similar method; they do not explore double differences, but they assess the impact of health insurance on public good contributions by comparing (predicted) adopters and non-adopters across villages with and without access to health insurance.

<sup>17</sup> Our method most closely resembles that applied by Coleman (1999). That is, we leverage the expansion strategy shared by the organization and estimate a double-difference model in space. However, Coleman has ex ante information about future adopters in the control region, whereas we lack this information and thus must estimate future adopters by using an out-of-sample forecast.

**Table 5. Explanation of Double-Difference Method**

	Kebeles with insurance access	Kebeles without insurance access
Non-adopters and (would be) non-adopters	C	B
Adopters and (expected) adopters	A	D

By comparing groups *A* and *D*, we can address selection bias at the household level. That is, the characteristics of participants and (expected) participants should be similar, so the difference between these two groups reflects the impact of insurance availability. Yet a simple comparison of the means obtained from groups *A* and *D* cannot address potential biases due to non-random program placement, which may be a serious issue if the order in which the insurance company serves the different areas is not random. Therefore, we use DiD to compare group *A* with *C* and group *D* with *B*, then take the difference between the two comparisons. Formally, the DiD estimate is  $(Y_A - Y_C) - (Y_D - Y_B)$ , where  $Y$  represents the outcome variable (e.g. access to credit), and the subscripts represent the groups.

We estimate this DiD estimator in a regression framework, using Equation 1 (including controls). Technically, the procedure ensures that unobservables related to treatment status and to the treatment area difference out. However, it does not difference out error terms that are specific to both adopter status and treated kebeles. It is straightforward to assert that a key assumption of our approach is that the difference between unobservables of adopters and non-adopters in the treatment area is the same as the difference between unobservables of would-be adopters and would-be non-adopters in the control area; we subsequently test this assumption using selection tests. If it holds, the coefficient for the interaction term ( $\beta_1$ ) provides an unbiased estimate of the impact of adopting index insurance on credit rationing.

Finally, because we *predict* adopters, it is important to note that  $D_i^t$  in Equation 1 is a generated regressor, which may lead to biased standard errors if we estimate the equation using OLS. Therefore, we use a bootstrapping

procedure to determine standard errors. Specifically, we wrote a stata.ado program that enables us to estimate Equation 1 simultaneously with the procedures to estimate expected adopters in the control kebeles (steps 1–3 in the next section), as well as bootstrap the entire process. More details (and the stata.ado file) are available on request.

## Results: Identifying Expected Adopters

A major challenge associated with our proposed method is the need to identify both expected future adopters and expected future non-adopters in the control kebeles. We identify expected adopters in the control kebeles by using estimates of the propensity to adopt from the treatment kebeles in a four-step procedure.

### Identification Procedure

*Step 1.* To estimate the propensity to adopt in the treatment area, we consider the decision to buy IBI. The utility difference of having IBI or not depends on the vector of characteristics  $Z$ . This utility difference can be written for each household as a function of observed characteristics  $Z$  and unobserved characteristics  $\varepsilon_{ij}$ . Assuming a linear additive relationship, we obtain.

$$(2) \quad D_i^{t*} = \beta Z_i + \varepsilon_i,$$

where  $D_i^{t*}$  is an unobserved latent variable. We assume the household buys IBI if the utility difference exceeds 0. Consequently:

$$D_i^t = 1 \text{ (adoption of IBI) if } D_i^{t*} > 0.$$

$$D_i^t = 0 \text{ (no adoption of IBI) if } D_i^{t*} \leq 0.$$

We also assume a logistic distribution of the  $\varepsilon_i$  values and estimate the equation with a binary logit model.

The results of estimating the determinants of IBI uptake using Equation 2 are in table 6. The estimates suggest that older (marginally significant) and female household heads exhibit a higher probability of adopting than younger and male-headed households. As we expected, the higher the *Basis risk*, measured by the distance of the household farm to the nearest metrological station measured in

walking hours, the lower the probability to adopt insurance. However, this effect is insignificant,<sup>18</sup> probably because of its high correlation with *Present-bias or procrastination*, measured by a binary dummy, equal to 1 for households that indicate that, if insurance is available, they would postpone the uptake decision until the final insurance sales day to make a better estimate of future weather conditions. This indicator measures whether households wait until the sales deadline, so that they can better forecast future weather conditions and then make their insurance uptake decisions.<sup>19</sup> Attending insurance promotion meetings positively affects uptake, according to the positive, significant coefficient for *Insurance product promotion* measured by a dummy variable equal to 1 for households that participated in a product promotion meeting (campaign) of JICA and OIC and 0 for those that did not participate. We consider *Education*, *Family size*, *Cooperative membership*, and *Draft oxen*, but only *Family size* and *Draft oxen* (marginally) are significant. The uptake equation thus appears able to separate adopters from non-adopters, as reflected by the area under the receiver operating characteristic (ROC) curve, which is equal to 0.84.

A crucial assumption of our proposed method is that selection effects due to unobservables can be controlled for by adding a dummy variable that indicates which farmers are adopters and would-be adopters. However, if our results turn out to be sensitive to a misclassification of (would-be) adopters, selection effects may seriously bias the results. To determine whether the results are sensitive to the precise specification of the uptake equation, we consider two alternative specifications, in which we add more variables to control for the potential omitted variable bias. First, we add three control variables: *Peer influence*, *Risk aversion (CRR)*, and *Time preference*. *Peer influence* is measured by a dummy, equal to 1 for households that indicate that their peers, relatives, or neighbors

who have bought insurance have influenced them to buy IBI, and 0 for others. In the expansion area, households indicated whether peer influence has convinced them to buy at the moment the insurance becomes available. For exact definitions of *Risk* and *Time preference*, see Table A1. Households may be more willing to adopt insurance if some of their peers already have bought it (*Peer influence*). *Risk aversion* likely negatively affects uptake, which may sound counterintuitive but is in line with studies that cite trust issues;<sup>20</sup> apparently, more risk-averse households do not trust insurance, so they are less willing to adopt it. Finally, the relationship between *Time preference* and uptake is positive<sup>21</sup> but insignificant.<sup>22</sup> Second, in another uptake regression, we ignore *Peer influence*, which could be affected by insurance: if somebody buys IBI, there are potentially more peers available that may positively affect other purchasers or non-purchasers. As we show in the online Appendix, Table S1 (Equations 1 and 2), the performance of the alternative uptake equations, in terms of R-square values and the ROC curve, are similar to that of our preferred uptake equation.

*Step 2.* Using Equation 2 and the estimation specified in table 6, we conduct an out-of-sample forecast to predict the propensity to adopt in the control area.<sup>23</sup>

*Step 3.* To identify expected future adopters and expected future non-adopters in the control area, we endogenously set a threshold value of the probability to adopt, above which a farmer is classified as an expected adopter according to the optimization of the so-called Youden Index (Youden 1950). This index estimates the probability of an informed decision, rather than a random guess and thus provides a measure of discriminatory power. It is calculated as  $sensitivity + specificity - 1$ , where sensitivity (or true positive rate) measures the proportion of correctly classified positives,

<sup>18</sup> *Basis risk* becomes significant if we drop the *Present-bias or procrastination* variable from the model. The main results hold for three alternative models: (a) both variables included, (b) only basis risk included, or (c) only *Present-bias or procrastination* included.

<sup>19</sup> In a given insurance sales period (e.g., 45 days), some households might wait to make their purchase decision until the 44<sup>th</sup> or 45<sup>th</sup> day, at which point they have the most updated information about future rainfall. Similar behavior, such that farmers update their rainfall beliefs in response to external forecasts, is documented by Lybbert et al. (2007).

<sup>20</sup> *Risk aversion* is derived from an incentivized lab-in-the-field experiment that we conducted to assess risk attitudes. It includes a multiple price list protocol that requires participants to choose between a safe and a risky option (50/50 probability; Binswanger 1980). More details are available on request.

<sup>21</sup> Our time preference indicator comes from a time preference game we played with all households in the sample.

<sup>22</sup> We do not drop insignificant variables from the uptake equations, so they still may affect the predicted outcomes.

<sup>23</sup> All variables in Step 1 are available for both the treatment and control kebeles, so out-of-sample forecasts are possible. This option also holds for the binary variable *Insurance product promotion*, because during our survey, the insurance company already had conducted promotional meetings in the expansion (control) kebeles.

**Table 6. Determinants of Uptake in Treatment Kebeles**

Variables	Uptake
Education (years)	0.028 (0.03)
Family size	0.120*** (0.038)
Coop member	0.218 (0.325)
Draft oxen	0.105* (0.054)
Basis risk	-0.0145 (0.042)
IBI product promotion	2.9260*** (0.250)
Age	0.011 (0.008)
Gender	-0.703** (0.290)
Present-bias or procrastination	0.717*** (0.188)
District 1	-0.294 (0.301)
District 2	-0.107 (0.235)
Constant	-2.868*** (0.527)
Pseudo R-squared	0.30
Area under ROC curve	0.84
Cutoff value for positive classification	0.626
Observations	812

Note: Cluster-robust standard errors are in brackets. The cutoff value for a positive classification is based on maximizing the Youden Index. \*\*\* $p < 0.01$ . \*\* $p < 0.05$ . \* $p < 0.1$ .

and specificity (or true negative rate) measures the proportion of correctly classified negatives. If the model perfectly predicts farmers with and without insurance, sensitivity and specificity take values of 100% and 100%. In turn, the Youden Index is defined between 0 and 1; it is equal to 0 if a test is deemed useless (i.e. the classification model gives the same proportion of positive results for farmers with and without insurance). For this study, we calculate specificity and sensitivity within-sample, whereas they are normally measured out-of-sample. We set a threshold value of 0.626 for our preferred uptake equation.

*Step 4.* We reclassify adopters in the treatment area, using the same method. Table 7 compares the percentages of predicted and actual adopters in the treatment area with the predicted expected adopters in the control area if we use our preferred uptake equation. The predicted percentage of adopters equals 60% for the treatment kebeles and 68% for the control kebeles.

### Selection Test (Balance Test)

To test the reliability of our method, we conduct selection tests that indicate if the selection

process is similar in the treatment and control kebeles. The selection tests require variables that have not been affected by the actual and predicted uptake of IBI (i.e., exogenous variables), so they should not include variables already in the uptake equation. The selection tests also need to consider characteristics of households that bought and did not buy IBI. We therefore address  $(Z_A - Z_C) - (Z_D - Z_B)$ , where  $Z$  represents exogenous variables and the subscripts refer to the different groups. In a regression framework, we estimate.

$$(3) \quad Z_i = c + \alpha D_i^t + \beta D_i^w + \gamma D_i^t D_i^w + \mu_i,$$

where  $\alpha$  captures differences between predicted adopters and predicted non-adopters, and  $\beta$  captures differences between the kebeles with and without access to IBI;  $D_i^t$  and  $D_i^w$  are as defined previously. The parameter of interest is  $\gamma$ , which captures differences between adopters and non-adopters beyond differences already accounted for by  $\alpha$  and  $\beta$ . An insignificant  $\gamma$  implies that the selection process in the two areas is similar in terms of observables. The selection balancing tests use the results from table 6.

As table 8 shows, with the exception of years of education, the selection tests suggest that all variables are balanced, which provides additional confidence in our approach and for our identification strategy. Adopters in the treatment region have slightly more education than expected adopters in the control kebeles.<sup>24</sup> To control for possible biases due to this difference, we add *Education (years)* as a control variable in the credit rationing regressions.

### Impact of IBI on Credit Rationing

We assess the impact of IBI on credit rationing in general according to a binary logit estimate and a linear probability model.<sup>25</sup> Table 9 presents the results; they suggest that farmers who adopt IBI are less credit rationed, as indicated by the significant negative coefficient for

<sup>24</sup> With large samples, a t-test (and our approach) may reveal imbalances, even if the differences are small. If the number of observations is very small, a null hypothesis of equal means may not be rejected, even if the differences are relatively big. Therefore, testing for balance requires consideration of the magnitude of the coefficient too. In our case, years of education is about 1–2 years longer for adopters in the treatment area, which seems relatively small. Imbens (2015) suggests using normalized differences as an alternative test for balance.

<sup>25</sup> Theoretically, IBI could induce lenders to reduce interest rates. However, we find no evidence of this action in our survey area. It appears that lenders do not properly price risk.

**Table 7. Adopters and Non-Adopters by Area**

	Treatment area	Percentage	Control area	Percentage
Actual adopters	459	56.5		
Predicted adopters	484	60	226	68
Actual non-adopters	353	43.5		
Predicted non-adopters	328	40	105	32
Total (adopters + non-adopters)	812	20	331	100

Note: In treatment area, 84% of treated are correctly predicted (sensitivity), and 73% of the non-treated are correctly predicted (specificity).

the interaction term. Its estimated parameter value is  $-0.957$ , indicating that adoption of IBI decreases the log-odds of being credit rationed by  $0.957$ , holding all other covariates constant.<sup>26</sup> Similarly, the OLS regression shows that IBI adoption decreases total credit rationing by 20%.

We further investigate the impact of IBI by differentiating between demand- and supply-side constraints. That is, to assess the impact of IBI on credit demand- and supply-side rationing, we use a multinomial logit model, with credit-unconstrained households as the base. Table 9 shows that uptake of IBI significantly reduces supply-side rationing. The estimated parameter value for the interaction term is  $-0.589$  for supply-side credit rationing, indicating that IBI adoption decreases the multinomial log-odds of being credit constrained (relative to credit unconstrained) by  $0.589$ . For a clearer interpretation of the impact of adopting insurance on being supply-side credit rationed, we exponentiate the coefficient ( $0.589$ ) to obtain the relative risk ratio, which produces a value of  $1.8$ . Therefore, the relative risk (or odds) of being non-credit rationed rather than supply-side credit rationed among IBI holders is about two times the corresponding relative probability for non-IBI holders with the same characteristics (e.g. education, age, family size). For the ease of interpretation, we also estimated a simple OLS specification for supply-side rationing. This regression suggests that the probability of being supply-side credit rationed is approximately 6% lower for IBI holders compared to non-IBI holders, which is lower than the 12% suggested by the endogenous selection model but still sizeable. Perhaps lending institutions extend more credit supply to insured households, which can earn indemnity payments from IBI that enhance their potential ability to repay the loan. Therefore, the loan applications of insured households may evoke better

acceptance among lenders. Although we also find a negative effect of IBI adoption on demand-side credit rationing, the impact is not significant at conventional significance levels.

### Limitations and Robustness Analyses

The main results suggest that adopting IBI causes farmers to be less credit rationed; uptake of IBI negatively affects both supply-side and demand-side credit rationing. However, only the impact on supply-side credit rationing seems significant. Our approach is based on some important assumptions though, including the prediction that selection effects due to unobservables can be controlled for by adding a dummy variable that indicates which farmers are adopters and would-be adopters.<sup>27</sup> Moreover, to determine the likelihood of being treated and non-treated, we estimate an adoption equation, but the coefficients in the uptake equation will be biased if the unobserved characteristics affecting adoption also affect being credit constrained and correlate with the observed characteristics in the adoption equation.<sup>28</sup> In such a case, our approach would be invalidated.

No straightforward method exists to test this underlying assumption, so we rely on a series

<sup>27</sup> An important underlying assumption is that after controlling for observables, unobservables generate little remaining bias.

<sup>28</sup> We thank a referee for noting that the difference between control and treatment kebeles, in terms of the degree of participation in the PSNP (Table 4), could reflect enhanced financial development in the control kebeles. This distinction could create an omitted location variable that affects both credit rationing and IBI adoption, which would confound our results and bias the estimation results. We cannot fully rule out this potential bias, but one of the main assumptions of our double-difference method is that the difference between unobservables of adopters and non-adopters in the treatment kebeles is the same as the difference between unobservables of would-be adopters and would-be non-adopters in the control kebeles. Even if some program placement variables are omitted, this assumption is not necessarily violated.

<sup>26</sup> The unit is the percentage change in the likelihood of uptake.

**Table 8. Selection Tests (Balance Tests)**

Variable	Education (years)	Family size	Extension contact	Coopmember	Dependents	Draft oxen
Treatment kebeles ( $D_i^W$ )	-0.52	-0.67**	0.05	0.02*	-0.42*	-0.33
Uptake ( $D_i^i$ )	-0.53*	1.33***	0.03	0.06***	0.80***	0.71*
$D_i^i D_i^W$	1.64**	0.31	-0.02	0.04	0.09	0.04
Constant	4.11***	6.53***	0.91***	0.01	3.43***	1.77***
Observations	1,143	1,143	1,143	1,143	1,143	1,143
R <sup>2</sup> adjusted	0.02	0.08	0.01	0.03	0.05	0.03

Note: Cluster-robust standard errors with kebeles as the cluster. \*\*\* $p < 0.01$ . \*\* $p < 0.05$ . \* $p < 0.10$ .

**Table 9. Impacts of IBI Uptake on Credit Rationing**

Credit rationing	Total credit rationing		Supply-side rationing	Demand-side rationing
Treatment kebeles ( $D_i^W$ )	1.241** (0.512)	0.274*** (0.100)	1.303*** (0.395)	1.147 (0.818)
Uptake ( $D_i^i$ )	0.841** (0.408)	0.177** (0.071)	0.339 (0.227)	1.365* (9.792)
$D_i^i D_i^W$	-0.957** (0.447)	-0.204** (0.084)	-0.589** (0.305)	-1.300 (0.863)
Constant	-2.252** (1.074)	0.006 (0.144)	-2.978*** (0.979)	-3.602 (2.607)
Controls	Yes	Yes	Yes	Yes
Estimation technique	Logit	OLS	Multinomial logit	
Observations	1,143	1,143	1,143	1,143

Note: Standard errors in parentheses are based on a cluster (kebele) bootstrap method, performed for a system of equations containing the uptake equation in table 6 and the credit rationing equation. The bootstrap relies on 750 repetitions. The controls are Age, Education (years), Family size, Extension contact, District1, District2, Dependency ratio, Dependents, and Cooperative member. \*\*\* $p < 0.01$ . \*\* $p < 0.05$ . \* $p < 0.1$ .

of robustness tests. We address five groups of potential biases: (a) misspecification of the uptake equation, (b) misspecification of the credit-rationing equation, (c) remaining program placement effects, (d) endogeneity bias and reverse causality, or (e) spillover effects from adopters to non-adopters.<sup>29</sup> With robustness tests, we can assess the impacts of these potential biases. Thus, many more regressions were run than can be included in the article. The interested reader can find them in a supplementary appendix online. The main results still hold. Yet we explicitly acknowledge that our results still might be due to unobserved factors that affect both credit rationing and insurance uptake. Therefore, we also conduct a placebo test, for which the findings still hold. The results of the placebo tests are also available in the Online Appendix.

## Conclusions

Index-based insurance promises a climate risk management strategy that can benefit the poor. This article focuses on a question that has not received much attention in prior literature: Does the adoption of index insurance improve access to financial markets and reduce credit rationing? Our empirical analyses focus on Ethiopia and leverage a situation in which IBI is not yet available in some areas. With a hybrid identification strategy, in line with a cross-sectional double-difference method, we try to control for potential selection biases by forecasting potential insurance adopters. With this identification strategy, we can draw some tentative conclusions about the causal relationship between adopting IBI and credit rationing. Overall, the OLS regression suggests that IBI adoption decreases the probability of credit rationing by 20%.

We also differentiate supply-side and demand-side forms of credit rationing. The impact regressions indicate that IBI reduces

<sup>29</sup> Because we ignore spillover effects, our estimates may reflect a lower bound.

both supply-side rationing (quantity rationing) and demand-side credit rationing, but the latter does not appear robust. The nonsignificant impact on demand-side credit rationing is not surprising as the insurance contract is a stand-alone product, for which indemnities are paid directly to farmers, and the majority of smallholders lack any valuable collateral to offer.

The study findings suggest a relatively large impact of IBI uptake on reducing supply-side credit rationing. Across various regressions, we find that the relative risk of being non-credit rationed rather than supply-side credit rationed among IBI holders is about two times the corresponding relative probability for non-IBI holders. Thus, IBI adoption appears to enhance smallholders' access to credit. It also provides mutual benefits to farmers (borrowers) and lenders. Alleviating supply-side credit constraints enables farmers to acquire inputs and enhance productivity. Because their access to credit overcomes their liquidity constraints, farmers can employ other risk management strategies to hedge against downside production risks. For lenders, lending to insured farmers reduces the default risk of lending. In presenting these outcomes, we take care to note that the findings are based on the results after one year of program implementation. It is not necessarily the case that lenders are more willing to provide credit specifically to insured applicants. Alternatively, increased lending could reflect decreased transaction costs when lenders combine credit supply with IBI provisioning. Further research is needed to determine the precise empirical implications.

Our proposed hybrid method can be used in settings in which an intervention already has taken place, such that no pre-intervention baseline survey is possible. Such a situation, which is common in reality, precludes either a standard double-difference method or a randomized controlled trial. Yet our proposed method is not without limitations. In particular, the ability to estimate adopters correctly and obtain unbiased estimates of the coefficients in the adoption equation is crucial. This method also relies on the important assumption that unobserved characteristics that affect both adoption and credit rationing are not correlated with observed characteristics that affect IBI uptake. There is no straightforward method to test this assumption, so we present various robustness analyses to increase confidence in the plausibility of our main results;

such analyses might not offer similar confidence in other settings. Because the robustness of our hybrid method approach has not been tested in alternative settings, it cannot offer an alternative to well-known, rigorous methods, like randomized trials. However, we hope this article encourages continued research that tests whether our method is appropriate in various settings, if our results hold in other settings, and whether the findings are robust to the use of other identification strategies, such as randomized controlled trials.

### Supplementary Material

Supplementary material are available at *American Journal of Agricultural Economics* online.

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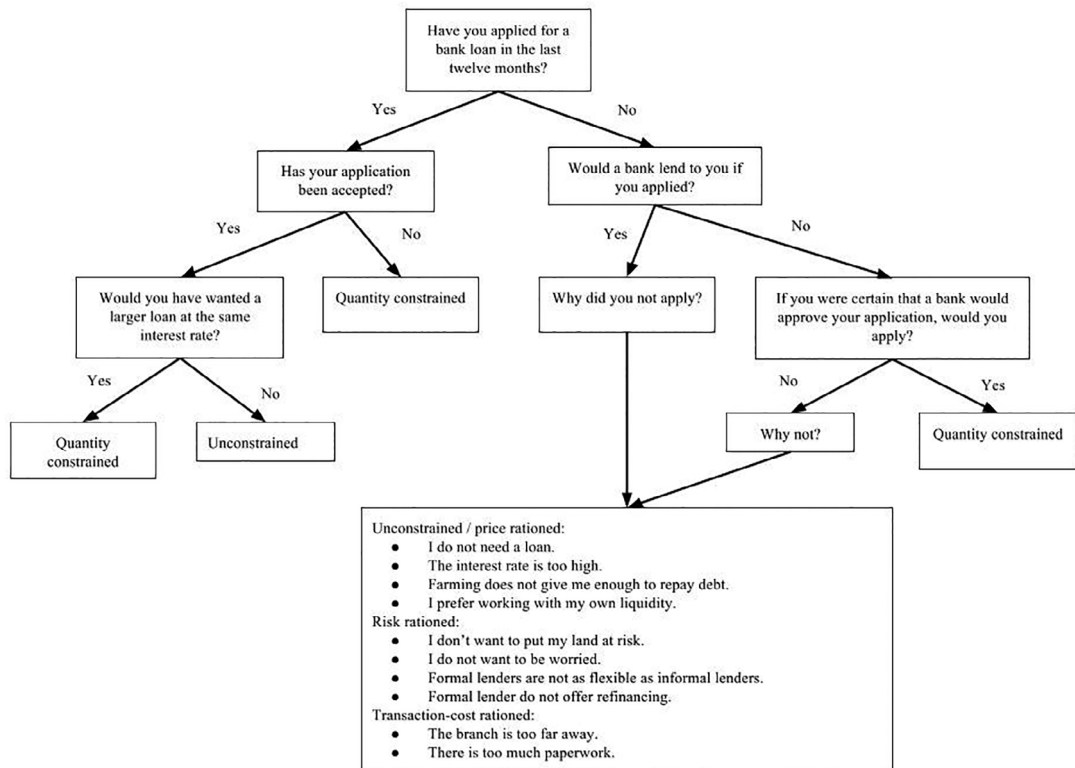
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## Appendix

### A. Tree diagram illustrating credit rationing status



**Figure A1. Direct elicitation method (Boucher, Guirkinger, and Trivelli 2009)**

#### Notes:

- Have you applied for a bank loan over the last five years?  
(1) Yes [n = 439].  
(2) No [n = 704]
- Has your application been accepted?  
(1) Yes [n = 383].  
(2) No [n = 56]
- Did you want larger loan at the same interest rate?  
(1) Yes [n = 57].  
(2) No [n = 326]
- If you did not apply, would a bank now lend to you if you apply?  
(1) Yes [n = 108].  
(2) No [n = 317].  
(3) I do not know (IDK) [n = 279]
- If you think that a bank would now lend to you if you apply, why do you not apply?

#### Indicate reason(s)

- (1) I do not need loan [n = 44].
- (2) High interest rate [n = 6].
- (3) Farming does not give me enough to repay a debt [n = 11].
- (4) I prefer working with my own liquidity [n = 4].
- (5) I do not want to put my collateral at risk [n = 13].
- (6) I do not want to be worried [n = 6].
- (7) I prefer informal lenders because formal lenders are so strict [n = 4].
- (8) Formal lenders do not offer refinancing [n = 5].
- (9) The branch is too far away [n = 6].
- (10) The lending procedures are too costly [n = 9].
- (11) IDK

- If you were certain that a bank would approve your application, would you apply?
  - (1) Yes [n = 121].
  - (2) No [n = 475].
  - (3) IDK
- Why not? Indicate reason(s)
  - (1) I do not need loan [n = 143].
  - (2) High interest rate [n = 56].
  - (3) Farming does not give me enough to repay a debt [n = 66].
  - (4) I prefer working with my own liquidity [n = 50].
  - (5) I do not want to put my collateral at risk [n = 53].
  - (6) I do not want to be worried [n = 13].
  - (7) I prefer informal lenders because formal lenders are so strict [n = 12].
  - (8) Formal lenders do not offer refinancing [n = 42].
  - (9) The branch is too far away [n = 17].

**Table A1. Variables**

Dependent variables	Variable type and description
Uptake	Dummy, equal to 1 for households that bought index-based insurance in 2013, 2014, or both; 0 for others
Credit rationing (1)	Dummy, equal to 1 for credit-rationed households and 0 for credit-unconstrained households
Credit rationing (2)	Categorical, equal to 0 for credit-unconstrained, 1 for credit supply-rationed, and 2 for credit demand-constrained households
Independent variables	
Risk aversion	Continuous, estimated as the value of constant relative risk aversion ( <b>CRRA</b> ) parameter based on the equation $u(\tau) = \frac{\tau^{1-CRRA}}{1-CRRA}$ , where $u(\tau)$ is the payoff (utility) of the safe option, and $\tau$ is the payoff in the form of the expected value of a risky option. Payoffs were constructed from a Binswanger (1980) lottery game that the households played with small, real incentives. We offered households a choice of a menu of real gambles, at varying levels of risk and expected payoffs.
Time preference	Continuous, an exponential discount rate estimated as the value of $\delta$ from the equation $PV = FV \left( \frac{1}{1+\delta} \right)^t$ , where $PV$ and $FV$ are present and future values, and $t$ is the discount period. Households were given a time preference game and chose from a fixed amount today ( $PV$ ) or a larger amount in the future ( $FV$ ).
Insurance product promotion	Dummy, equal to 1 for households that participated in a product promotion meeting (campaign) of JICA and OIC and 0 for those that did not participate. In the expansion area (control kebeles), product promotion campaigns have taken place.
Peer influence	Dummy, equal to 1 for households that indicate that their peers, relatives, or neighbors who have bought insurance have influenced them to buy IBI, and 0 for others. In the expansion area, households indicated whether peer influence has convinced them to buy at the moment the insurance becomes available.
Basis risk	Continuous, distance of the household farm to the nearest metrological station measured in walking hours.
Gender	Dummy, equal to 1 if head of the household is male, 0 otherwise
Present-bias or procrastination	Binary dummy, equal to 1 for households that indicate that, if insurance is available, they would postpone the uptake decision until the final insurance sales day, to make a better estimate of future weather conditions.
Draft oxen	Number of draft oxen that the household uses for production
Education (years)	Household head's level of education in years of schooling
Distance from market	Distance from market
Land size	Household's land holding, measured in a local unit called qarxi, where 1 qarxi = 0.25 hectares
Extension contact	Dummy, equal to 1 for households that frequently make contact with extension agents; 0 for others
Dependents	Number of dependents in a family
Dependency ratio	Dependents/family size
Additional variables	

*(Continues)*

**Table A1. Continued**

Dependent variables	Variable type and description
Population density	The ratio of the households' family size to land size.
Housing quality	An indicator variable indicating whether the household has an iron corrugated house
Livestock owned in TLU	The size of livestock owned by the household measured in TLU, which is a standard unit of measurement.
Access to non-farm employment	Indicator variable equal to 1 if a household has non-farm employment
PSNP dependent	Indicator variable equal to 1 if the household is a beneficiary of a productive safety net program
Net salvage value of physical assets	Current value of the household's usable assets as estimated by the household
Saving	Household monetary and non-monetary (cereal storage) savings in Birr
Wealth index	An index constructed on the basis of information from various plant asset compositions owned by households and the salvage values of each asset in our data set.

**Table A2. Additional Descriptive Statistics**

Variables	(1) Family size	(2) Coop member	(3) Peer influence	(4) Basis risk	(5) Draft oxen
Treatment kebeles (adopters)	7.3	0.1	0.9	2.5	2.1
Treatment kebeles (non-adopters)	6.2	0.1	0.6	2.5	1.6
Control kebeles	7.4	0.1	0.5	1.3	2.3
Observations	1,143	1,143	1,143	1,143	1,143

*Note:* The statistics in Table A2 refer to the mean of uptakers in the treatment kebeles; the mean of non-uptakers in the treatment kebeles; and the mean of households in the control (expansion) kebeles.